

PUBLIC HEALTH LABORATORY SERVICE  
(Directed by the Medical Research Council for the Ministry of Health)

Central Enteric Reference Laboratory and  
Bureau,

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16th December, 1953.

Dear Dr. Lederberg,

Please forgive me for not having written before to thank you for your letter of the 4th November. I have been away for a short while and have had various preoccupations, and this was the reason for the delay in writing to you.

(1) I very much appreciate the way in which you have replied to the several questions raised in my previous letter. Paragraphs 1 to 6 of your letter contain a wealth of evidence and I have to admit that the evidence appears to be unassailable. That is to say, for the time being one has to accept the conclusion that bacteriophage particles are themselves transducing particles, and wait to see whether or not the two will be separated in the future.

What was quite new to me was the treatment with alcohol mentioned in your paragraph 2. Has this been published already? And if so, would you be good enough to give me the relevant reference?

The neutralization experiments referred to in your paragraph 5 are also most interesting and important.

(2) I have no doubt whatsoever as to the correctness of the serological typing of the various Salmonella strains employed in your experiments, and I could see no point at all in re-examining the original or the newly altered types.

(3) I had a talk with Dr. Spicer, as you suggested. He seemed to be aware of the possible complications that might in the future arise from the use of the term "transduction" in respect of both, the homologous and the heterologous antigens concerned in experiments with phage lysates. However, he appeared to think of this risk as something of a remote possibility, whereas I am looking upon it as a real danger that may very soon confront workers engaged in enteric phage typing. Your recent paper with Edwards in the Journal of Immunology, which you have mentioned but which has not yet arrived here, and the paper with Stocker will certainly

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make a great impression not only on research workers but also on less competent bacteriologists throughout the world. As you know, non-motile variants, temporary O forms, are often found on primary isolation, especially now that the modern enrichment media are employed, and the thoughtless routine bacteriologist will immediately look for help from your transduction technique.

It seems to me to be highly desirable that you and your collaborators take steps at an early stage to avoid confusion which seems to be imminent. Whatever may be the fundamental theoretical aspects that still await elucidation, from the practical point of view I think it would be important to distinguish from the outset between the two phenomena. If the unmasking of the inherent homologous antigens were, for the time being, referred to by some distinctive term, for instance, "induction", in contrast to "transduction" of heterologous antigens, that would be for practical purposes a sufficient safeguard against possible confusion. I wonder what you think of this suggestion?

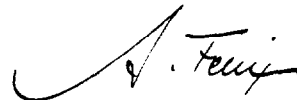
(4) I was delighted to read on page 4 of your letter (paragraph 4), of the transducing genetic material being "inaccessibly embedded in the phage particle." This reminded me of our earlier correspondence and I looked up your letter of the 5th January, 1953, the last sentence of which read as follows: "If all phages, and not merely lambda, prove to have unique localizations on bacterial chromosomes, I would have to admit that they have not, in fact, evolved in this way."

I know you will say old Felix is always coming back to his hobby, but I could not resist the temptation. Now that the endogenous origin of phage is being gradually re-introduced, I may perhaps live long enough to receive one day even some credit for having upheld my unorthodox views almost single-handed.

As you see, I do derive genuine satisfaction from our correspondence.

With kind regards,

Yours very sincerely,



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*Best wishes for 1954.*

*Replied  
reprint*